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THE TRUE AND THE FALSE IN DARWINISM.

CRITICAL REPRESENTATION OF THE THEORY OF ORGANIC DEVELOPMENT. BY EDUARD VON HARTMANN. BERLIN, 1875.

TRANSLATED FROM THE GERMAN BY H. I. D'ARCY.

III. The Theory of Heterogeneous Generation, and the Theory of Transmutation.

[Continued from JOURNAL OF SPECULATIVE PHILOSOPHY for April, 1878, and July and October, 1877.]

This is the case, for instance, with the fresh-water snail, *planorbis multiformis*, found near Steinheim (conf. Ph. d. Unb. 8ter. Ausg. p. 594), the form-circle (*formenkreis*) of which, shifting between very distant limits, shows uniform systematic transitions in all directions; but yet, with the exception of just those forms, which, like *denudatus* or *trochiformis*, might manifest a tendency to the type of a new species or genus, and which, in accordance with the theory of heterogeneous generation, make their appearance suddenly; again, as regards the forms related to each other by transition, at least as great variations are to be found among those of the same period — that is, those deposited in the same horizontal stratum — as among the oldest and the most recent stratum, so that the geological features present, on the whole, the appearance of a species with complicated extensions forward, backward, and sideward, but still confined within a definite circle; they afford no evidence favorable to the gradual transmutation of one species into another.[1]

Since, then, embryology and paleontology seem rather to oppose than to favor the theory of transmutation, the latter sees itself forced to seek its support in the materials drawn from the present fauna and flora. It would be a very appropriate task for a theory of natural science to strengthen its assumption of the descent of all organic bodies by means of gradual transmutation, since such assumption transcends experience, from the analogy of some processes, however few, of transition, experimentally established, of one species into another. Darwinism must, however, admit that it has not yet been able to fulfill this condition, and that it continually requires us to regard the transition shown by artificial grouping as a genetic transition. Even in artificial breeding it has not yet succeeded in procuring a pigeon which, with every external variation, does not retain the decisive specific characteristics of the pigeon. Now, the more efficient the means at the command of the breeder, compared to those of nature, the less favorable would the contrary result of artificial breeding be as evidence of natural processes in the origin of species; therefore, the above-mentioned negative result must present the transmutation theory in a rather suspicious light. But as we cannot have recourse to any direct observation of the origin of a new species, nothing remains but, in order to secure a ground for wider analogies, to select such varieties as at first view seem to lead, through a gradual intensifying of their variations, from the original form to a new species.

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[1] Compare Wigand's thorough criticism (No. 14 of the appendix) of Helgendorf's monograph. Wigand's results are completely confirmed by an examination of paleontological materials by Sandberger, of which he seems to have been ignorant (Verhandl. der Physik. med. Ges. zu Würzburg, N. F. B. d. V. S. 231). Sandberger refers in support of his own views to Hyatt of Boston, Leydig, and Weissman.

Varieties can be divided into three classes: First, those in which only the color, hair, texture, thickness of the cell walls, chemical composition, etc., are affected; these can be affected, partially at least, during the life of an individual, by a change of surroundings (local varieties), but are even in those instances, when they seem to appear spontaneously, not at all calculated to establish systematic differences. Second, monstrosities. Third, morphological varieties. (Wigand, p. 48 to 52.) In the case of monstrosities, we should distinguish those where there is a retrograde metamorphosis from those where such metamorphosis does not occur. The former, which are chiefly found among domesticated creatures, display, according to rule, a luxuriance of growth at the expense of sexual power, and at the same time a descent to a lower morphological and physiological level of organization, and should, therefore, be excluded in our consideration of the ways and means through which the true ascending development of organic bodies is affected. We are, therefore, really led to the monstrosities where there is no retrograde metamorphosis, and to morphological varieties; and for our purpose each of these, in a certain sense, completes the other. The morphological variety presents a perfectly complete type, without any extraordinary characteristic, but just for this reason the degree of departure from the type of the original form is not so striking as to warrant the conclusion that the character of the species has been lost. With the monstrosity, on the other hand, this loss of specific character is obvious, but only in the direction of some one particular feature. This feature often deviates so far from the form-circle of the species that it seems morphologically like the type of a different genus, or even family; but it does not lead to a new and complete type, for such would require a whole series of successive correlative changes.

We can, therefore, for the present take any one of the following views as to the origin of species: That monstrosities remain, and the other characteristics are acquired by degrees in the same way; or that morphological varieties extend further in the same direction in which they have deviated from the parent form; or that the result of each process is simultaneously reached — that is, the typical completeness of the morphological variety and the sudden variation of the monstrosity. Whatever view we take, we still have to deal with abrupt changes. While all varieties which result, not from the influence of external circumstances on actual individuals, but from spontaneous change in generation, emerge at once before our eyes, the suddenness is peculiarly striking with which monstrosities, not only in artificial life, but even in nature, — and, therefore, independently of external influences, arise spontaneously, — come into existence complete, and, *per saltum*, as something entirely new (Wigand, p. 50). Upon this phenomenon Hofmeister based his theory of the origin of a new species. (*Handbuch der physiologischen Botanik*, 1, 563, 564.) We may, indeed, define monstrosity as a partial heterogeneous generation in a different way, but the single steps of the process always remain so long that they are quite inconsistent with the transmutation theory, which, in a strict sense, requires changes so slight as to be inappreciable. Even if a species might, during a very long period, complete its form-circle, though moving with inappreciably short steps, still, experience shows that the really decisive steps which introduce something morphologically new can be traced within the species; and we should have much less reason to doubt that, in the great majority of cases of transition from one species to another, such a step over a greater or a less interval is requisite.

If we bear in mind what has been already said, it is clear that we shall find ourselves forced, for many reasons, to assume that the interval between two types connected by descent is crossed *per saltum*, whether the interval is crossed by a single leap or the process is regarded as one made up of several steps. This division of the process may occur in very different ways, as the metamorphosis of animals, alternate generation, dimorphosis, monstrosities, or morphological varieties; but always the least change from one variety of the same form-circle to another in the case of a morphological variety, which is characterized by an addition to its organs, or by the increase or diminution of the numerical relation of its parts, is only conceivable through a germ-metamorphosis, which introduces the change of type by a morphologically different arrangement of cells in the embryo.

As far, however, as the transmutation theory is concerned, the foregoing observations in no way affect its operation, in so far as this is limited in assisting in the development of specific types in their shifting form-circles, and in supplying a broader basis, and lessening the intervals to be crossed by heterogeneous generation, and so reducing each interval to a minimum. On the other hand, it would be very difficult to prove the assertion that any species has actually originated by simple transmutation from its direct ancestors. It cannot, under the circumstances, be denied that it is possible that nature may have in every case availed itself of heterogeneous generation. Indeed, if the older school of natural philosophers was right in maintaining the constancy of species, it would be hazardous to assert that species could possibly originate by mere transmutation. I believe, however, that I must regard the establishment of the changeable nature of the limits of species hitherto assumed to be unchangeable, and the proof that the permanance of species, like that of human characteristics, has only a relative meaning within certain limits, as one of Darwin's chief services, and as the one whose value will be longest recognized. Those interested in Wigand's book must, therefore, regret that it has made an unsuccessful attack upon this very position, and thereby exposed a weakness to the followers of Darwin which they will scarcely fail to see and utilize. But as the principle of the transmutation theory extends beyond the form-circle of the species to the theory of descent itself, and as this principle stands and falls with the mutability of species, we must look for a moment at this latter question.

That the conception of species is no more a fiction than any other abstract conception, but is founded in the nature of individuals, is freely admitted; it, however, ascends from the conceptions of orders, families, etc., and descends from that of the variety. It is not denied that these collections of common characteristics are founded on the nature of actual individuals; it is only denied that these systematic classifications have steadfastly fixed limits. When we have classified a particular domain of the natural system, and arranged it in a succession of groups, of which each higher one includes a number of lower ones, it still is for each one of us to decide, unless opposed by a long-reaching and uniform custom, which of these groups will receive the name of a species; and the extraordinary difference of opinion among natural philosophers as to the classification of species in most of the domains of the natural system best shows how difficult it must be to ascertain objective *criteria* wherewith to connect and reconcile conventional definitions.[2] Whoever, then, will endeavor to attack this shifting meaning of the conception of species as really unfounded, will naturally, in the first place, labor to discover an absolute criterion for this conception. Wigand thinks this criterion is best supplied by the phenomena of crossing. He admits that there are different species which produce fruitful offspring, but he denies that this crossing can produce fruitful and lasting results; and he accordingly asserts that we have in this, at least, a negative mark of species. That is, if two forms do not cross so that their offspring will be perfect and fruitful, this is decisive that such forms belong not simply to different varieties, but to different species (p. 31), and Wigand, therefore, defines his test of perfectly fruitful crossing as "certain and easy impregnation, perfect fruitfulness, and such a constitution in the first and all succeeding generations as precludes the possibility of a retrogression to the ancestral form" (p. 29, note). Each of these three conditions is, however, incapable of fulfillment even within the limits of a single species; its non-fulfillment, then, can by no means prove that two forms do not belong to the same species. If impregnation within the limits of a species were certain, married women would be always pregnant; if all offspring were fruitful, none would be unfruitful except those produced by crossing; finally, if all retrogression were excluded, all the species among which atavism occurs must be declared to be themselves the products of crossing. The criterion, therefore, of perfectly fruitful crossing goes far beyond the mark when it undertakes to establish a relative

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[2] Ernst Häckel's monograph on "die Kalkschwämme" (Berlin Reinner, 1872), vol. 1. "Biologie der Kalkswämme," pp. 474-478, affords a striking example of

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this. Häckel comes to the following result: "The natural system may, for instance, underlie the six following combinations: A, 1 gen. with 1 species; B, 1 gen. with 3 species; C, 3 gen. with 21 species; D, 21 gen. with 111 species; E, 43 gen. with 181 species; F, 43 gen. with 289 species. On the other hand, the artificial system admits of the six following groupings: G, 1 gen. with 7 species; H, 2 gen. with 19 species; I, 7 gen. with 39 species; K, 19 gen. with 181 species; L, 39 gen. with 289 species; M, 113 gen. with 591 species. Each of these twelve systems could advance plausible claims for itself, as each system-maker renders them prominent in support of his own principle. None of them, however, could ever be shown to be the absolutely true system." P. 477. The note on page 478 gives a more accurate account of these systems and of the different principles adopted in each.

degree of fruitfulness within the species as an absolute test (Conf. Ph. d. Unb. 8ter. Ausg. pp. 591–592), and if this criterion is only a relative one, it is a mere question of degree — that is, it is a question of fixing conventional limits for a sphere which, from its very nature, cannot be strictly limited.

Another remark made by Wigand, though rather incidentally, by which he associates species with the highest point in the curve described by fruitfulness, seems of more importance. The sexual affinity is greater between two different blossoms of the same tree than between the pollen and organs of one and the same blossom (on this account measures have been adopted, in the case of several plants, to prevent their self-fecundation); greater between two different individuals of the same form than between two different blossoms of the same tree, and greater between two varieties of the same species than between two similar individuals. But, on the other hand, fruitfulness rapidly decreases after the limits of species have been passed. In opposition to this, we must observe, firstly, that the decrease of fruitfulness with the increase of intermixture, though true of certain species, is by no means an universal law; and, secondly, that the maximum of fruitfulness, the highest point in the curve, on which Wigand lays so much stress, is frequently not to be found in the species, but in the variety. In a large number of plants, impregnated by pollen carried by the wind, and as well as in some others, self-fecundation may be regarded as the rule. It must, therefore, suffice for the preservation of the species; or, according to Wigand's unfortunate terminology, be perfect. In the case of gregarious animals which have polygamic habits the intermixture is also perfect, and does not occasion the disadvantages which always follow in its train when artificial breeding is resorted to. When varieties diverge widely from each other, they often manifest a decided objection to crossing; they will at least give the preference to individuals of their own variety. It is even asserted by many observers that in some instances varieties are less fruitful when crossed than, in other instances, species are.

We may, therefore, conclude that, in many instances, the highest point of the curve of fruitfulness does not coincide with species, but lies within this, upon the variety; or, perhaps, within still stricter limits.

We may, however, fairly assert that species is seldom far from the maximum of fruitfulness; and this may be a very important point of view, as a relative criterion for the empirical decisions as to what species is and what it is not. We may, perhaps, assume that in such cases, where a clearly defined maximum of the curve exists, this actually corresponds with the species, provided that the species has left its originating process behind, and no new process of specific development within itself has commenced. If the species has not yet come to a stand-still — if it is not yet completely established — there is still a certain tendency to cross with allied species, from which it is separated by more or less indeterminate boundaries; but if, on the other hand, a new process of specific development has begun, if its varieties are already so sharply defined that one might doubt whether he should regard them as species, then the maximum of fruitfulness has generally been transferred to the varieties.

The circumstance that, as well as the developed species, we also find undeveloped and over-developed species which still remind us of varieties; such as include within themselves varieties which resemble species, speaks most distinctly for the mutability of actual species, even for the proposition that the conception of species in the sense of the developed, and not yet over-developed, species coincides with the highest point of the curve of fruitfulness.

Whether, however, such a point exists, and how we, where direct observation of fruitfulness is impossible, should apply this criterion in the determination of species, remains now, as before, undetermined.

Against the evidence adduced in favor of the mutability of species, a reference to the constancy of species during the period over which our experience extends is, of course, of no avail — at least, if it depends exclusively upon perfectly developed species. The fact that this or that species has remained constant since the building of the Egyptian pyramids cannot prove that now certain divergent varieties are not about to acquire the character of species, or that certain undeveloped and shifting species are not tending to develop and establish themselves. The time within which attention has been directed to these processes is really too short to expect conclusive results from them. We are inclined to conclude as to the course of the development process, from the few different phases of it which lie before us, just as we conclude from the gaseous, glowing cloud-streaks, the burning liquid suns, and the solid moons, as to the whole cosmic development of these bodies.

Wigand says (p. 30): "Therefore the absence of transition is by no means a decisive criterion of species, since there are varieties in which no transitions occur; if, however, a transition is shown from one of two given forms to the other, this is conclusive proof that these are not different species. The constancy of form during reproduction, and under all circumstances, is not an unerring sign of species, because varieties manifest, in a measure, a similar constancy; but a form which, under a certain change of circumstances, or in the course of time, changes into another form, or is demonstrably generated from another form, is not specifically different from this other form." These positive criteria as to what forms are not to be regarded as distinct species do not, after what has been before said, require further refutation. Varieties which already appear constant should be regarded as inchoate species, and if it were to happen in the course of time that we should observe the growth of new species in this way, it would be entirely erroneous, in reliance upon the prejudice in favor of the constancy of species, to deny these the character of species, instead of recognizing the thus established mutability of species in the development of organic types. For the moment, the only object is to establish transitional forms, although, of course, these cannot be found between those species which have originated from varieties between which, as varieties, transitional forms did not exist. But even if transitions should be discovered between two forms which hitherto had been regarded as species, it would be premature to cry out, "then there are no species;" such an instance, and their number is constantly increasing, would rather suggest a new reason to approach the correction of the old notions as to the constancy and absolute independence of species. Wigand himself shows the worthlessness of this test (p. 18), and even maintains that "the form-circle of one species may touch that of another," by which the much dreaded transition is established.

The theory of the mutability of species, while being developed, is supported by the fact that we cannot find among the oldest fauna and flora representatives corresponding to the species which exist to-day, while we can find such representatives of the genera (*Gattungen*), families, and orders; that, moreover, the paleontological representatives of the present forms are decidedly less different from each other than are the latter; that, for instance, the representatives of families at an early geological period are only distinguished from each other as genera, and at a still earlier period as species. Even when we look at different divisions of the animal kingdom, — for instance, at the fishes and the amphibious animals, — we arrive, by going backwards, at a time when the average difference between them becomes continuously less. Wigand disputes this fact also; and, although he cannot fairly deny the decrease of difference as we go backwards, yet asserts that the different systematic characteristics are distinguished from each other, not only in degree, but in kind; so that, for example, two species could never come from two genera. But, unfortunately, Wigand is not in a position to state wherein lies the exact difference between the idea of species and that of genus; and as he cannot do this, we must retain the assumption that these ideas are distinguished only by the degree of difference, which degree is clearly sufficient for a continuous development. According to Wigand's own opinion, there is nowhere in the natural system so complete a difference as between the variety and the species; if, then, we have recognized this as one of degree, the same must certainly hold good of all other differences. If the idea of species lies in close proximity to the highest point in the curve of fruitfulness, this shows nothing more than that a certain combination of agreement and difference is most favorable to reproduction; if the point of difference advances, then the most favorable relation between agreement and difference must be sought at a point further back, — that is, the process of differentiation at this forward point, developed differences which require for their characterization a higher systematic mark than that of species.

It follows, therefore, that the proposed test of species —the maximium of fruitfulness — does not at all afford such a criterion for the difference between species and genus as would prevent the progress from one to the other in the advancing process of differentiation. Only thus much is true in Wigand's argument against the mutability of species that every species is not capable of change, but only such as in its morphological divergence from a genealogical ancestor, carries within it the tendency to further morphological development; and the broader the types which further development of the species introduces, the more essential is the fulfilment of this condition. The more striking the new morphological element exhibited by such a species in its organic development, the higher the degree in which it is qualified to serve as the first parent of a new order or class, the more certainly necessary is an act of heterogeneous generation, and the more powerless must mere transmutation appear.

What we have thus gained for the transmutation theory by the recognition of the mutability of species is nothing more than that we have given to it what had been completely taken away by the doctrine of the constancy of species, — that is, the possibility of explaining the transition from one species to another, when these do not manifest such great morphological differences that a retrograde form-metamorphosis becomes necessary. We have by no means, however, received for the transmutation theory more than the mere possibility of such explanation, and this possibility can only become a probability in actual cases when the probability is established that the regular series of intermediate forms between undoubted species is a genealogical series; the certainty of this could only be shown by observation of a process of transmutation occurring before our eyes. It will be seen that the transmutation theory stands on very weak supports, whether the mutability of species be admitted or not; while everything advanced above against its correctness, and in favor of the theory of heterogeneous generation, remains entirely unaffected by the question of the constancy or mutability of species. The result, then, of this chapter is, that even if future discoveries and observations should give a larger sphere to the operation of transmutation than it can claim in the present condition of our knowledge, yet the construction of the very foundation of the natural system will devolve upon heterogeneous generation, and the function of transmutation will be rather to clothe the skeleton with flesh and skin, to aid the evolution of variety in the domain of organic forms, and at the same time to prepare the way for further heterogeneous generation. Both are simply means by which the inherent law of development manifests its operation, and both mutually support and supplement each other. It is entirely erroneous to suppose that the one theory excludes the other; the only question is as to the relative extent of their influence and the limits of their operation. If, however, it were necessary that one of them should be excluded, then the construction of the organic world, by means of heterogeneous generation without transmutation, would seem to be at least quite possible, and the construction of it by means of gradual transmutation without heterogeneous generation would appear to be utterly impossible. The disputed point is, however, that Darwinism maintains that this impossibility is the truth, while the advocates of heterogeneous generation, on the other hand, by no means assume so hostile a position as to the coöperation of transmutation, but rather concede to it a more or less extensive influence. We must, therefore, conclude that the non-Darwinian advocates of the theory of descent are at least much nearer the truth than is Darwinism, in its exclusiveness as regards heterogeneous generation.